

**BELDOCK LEVINE & HOFFMAN LLP**  
**99 PARK AVENUE, PH/26<sup>TH</sup> FLOOR**  
**NEW YORK, N.Y. 10016**

JONATHAN MOORE  
DAVID B. RANKIN  
LUNA DROUBI  
MARC A. CANNAN  
CYNTHIA ROLLINGS  
JONATHAN K. POLLACK  
HENRY A. DLUGACZ  
STEPHEN J. BLUMERT  
MYRON BELDOCK (1929-2016)  
LAWRENCE S. LEVINE (1934-2004)  
ELLIOT L. HOFFMAN (1929-2016)

TEL: (212) 490-0400  
FAX: (212) 277-5880  
WEBSITE: blhny.com

COUNSEL  
BRUCE E. TRAUNER  
PETER S. MATORIN  
KAREN L. DIPPOLD  
MARJORY D. FIELDS  
EMILY JANE GOODMAN  
JUSTICE NYS SUPREME COURT, RET.  
FRANK HANDELMAN

REF:  
900000.03700  
WRITER'S DIRECT CONTACT:  
212-277-5850

December 2, 2021

**VIA ECF**

Hon. Analisa Torres  
United States District Court  
Southern District of New York  
500 Pearl Street  
New York, NY 10007

Re: *Floyd, et al. v. City of New York, et al.*, 08 Civ. 1034 (AT)  
*Davis, et al. v. City of New York, et al.*, 10 Civ. 0699 (AT)

Dear Judge Torres:

On behalf of Plaintiffs in the above-referenced matters, we write in response to the Fourteenth Report of the Independent Monitor, *Floyd* ECF No. 863-1, filed on October 12, 2021 (the "Report"). Specifically, we wish to bring to Your Honor's attention several concerns we have with the Report. Our concerns arise from failings of the Report that we fear may go unresolved in future reports, just as they went unresolved after our letter responding to the Fifth Report. *See* Letter to the Court of June 7, 2017, *Floyd* ECF No. 556. Since these concerns arose out of discussions with *Floyd* Plaintiffs' experts Professors Jeffrey Fagan and Jack Glaser, who found the Report to commit a variety of serious statistical errors, we have included their declarations as exhibits to this letter.

We detail our concerns more fully below but summarize them here for convenience:

- The use of calls for service ("CFS") as a benchmark for social distancing violations is unreliable and deviates from widely accepted statistics practices, since there are many reasons to doubt that CFS are representative of the actual rate of violations.
- The Monitor makes inaccurate statements about "enforcement" while excluding from its definition the very form of enforcement this monitorship is meant to address—police stops.

BELDOCK LEVINE & HOFFMAN LLP

Hon. Analisa Torres

December 2, 2021

Page 2

- The Report commits a variety of statistical errors in analyzing the relationship between race and enforcement, including failing to control for other factors that affect rates of enforcement.
- Almost all of the Report's conclusions about the absence of racial bias in enforcement are based on inadequate data or faulty inferences and are likely to reflect false-negative errors.
- The Report's conclusions about the constitutionality of enforcement are based on too small a sample of body-worn camera ("BWC") footage and rely on unverified statements from the Monitor about what that footage shows.

Each of these problems compounds the others, resulting in a report that is misleading and which lacks the analytical rigor required for the Court to credit its conclusions. Most concerning is the Monitor's consistent failure to recognize the possibility of racial discrimination where disparity exists. Since the Court will eventually be relying upon the Monitor's reports and representations in determining whether the NYPD is in compliance with the liability and remedial orders, we want to ensure that the Court is aware of these serious methodological problems. If they persist in future reports, we fear they will undermine the Court's ability to reach accurate and reliable determinations regarding substantial compliance.

### **1. CFS is a Flawed and Unreliable Benchmark for Social Distancing Violations**

As a foundational matter, the Monitor improperly uses 911 and 311 Calls-for-Service (CFS) as a benchmark to estimate and measure the total number of observable social distancing violations. *See* Report at 10. One major problem with using CFS as a proxy for violations is that their number and distribution are responsive to many factors other than the actual rate at which mandates were violated. *See* Exhibit 1, Declaration of Jeffrey Fagan ("Fagan Decl.") at ¶ 4. Indeed, the Report itself lists other variables that influence CFS levels, including "population density," "the proclivity of people to call the police and report social distancing violations," and "the overall rate of COVID-19 infections in a precinct." Report at 37. Each of these factors, taken alone and especially when combined, could substantially affect the accuracy of using CFS to represent the rate of social distancing violations.

In addition to being influenced by non-behavioral factors, CFS are also likely overinclusive. Since they are not verified for accuracy, CFS could include calls from people looking merely to harass their neighbors, people who may have misapprehended the distance between persons on a street, as well as people who misunderstood the social distancing rules. In contrast, crime reports are generated by police, or by crime victims, based on narrow definitions of behaviors describing discrete events and often verified by police. *See* Fagan Decl. at ¶ 5. The Report also does not account for the possibility that several people may have called for service regarding the same purported violation, which could produce multiple CFS for a single violation.

BELDOCK LEVINE &amp; HOFFMAN LLP

Hon. Analisa Torres

December 2, 2021

Page 3

Finally, the unreliability of CFS as a baseline is obvious even from the Monitor’s analysis of BWC footage. According to the BWC analysis, a majority—68 percent—of the social distancing arrests in a random sample of 78 resulted from encounters that were initiated by officers, not by civilian reports. *See* Report at 28. Yet CFS, as we understand them, do not include encounters that are initiated by officers during routine patrols, radio runs, or calls for other infractions, but are instead limited to calls placed by civilians or complainants. *See id.* at ¶ 5; *see also* Exhibit 2, Declaration of Jack Glaser (“Glaser Decl.”) at ¶ 7. Since the officers initiated a substantial number of encounters, presumably on the basis of observed violations, CFS, which exclude these encounters, cannot be relied upon as an approximation of the total number of violations. Even more troublesome, in its estimation of the probability of enforcement, *see* Report at 10, the Monitor included those 78 arrests in the numerator of “enforcement actions”<sup>1</sup> but did not include the officer-initiated encounters from which they arose in its denominator. In other words, officer-initiated encounters that translated into an arrest were accounted for as enforcement actions but not as corresponding violations. This decision is not consistent with the NYPD practice of recording arrest-generated crimes in crime statistics and reporting. The counting system, which ignores officer-generated crimes, will in turn overestimate the probability of enforcement.

For these reasons, any external peer reviewer would challenge the validity of using CFS as a benchmark for violations and would question the reliability of the Report’s resulting calculation of the probability of enforcement. *See* Fagan Decl. at ¶ 4. Since it serves as a foundation for much of the Report, the Court should approach any analysis that relies on this benchmark with skepticism.

## **2. The Report Makes Inaccurate Conclusions about Rates of Enforcement While Excluding “Stops” from its Definition**

We are also concerned with the Report’s omission of police stops from its analysis of race and enforcement in Part IV and from the definition of “enforcement” that it uses throughout. *See* Report at 10 (defining “total enforcement” to include only “arrests and summonses”); *see* Glaser Decl. at ¶ 6. This Court has always viewed stops as a form of enforcement. *See* Liability Opinion at 67 (including stop numbers in the definition of “enforcement activities”); *id.* at 71 (referring to quotas for “stops, arrests, or *other* enforcement activities” (emphasis added)). Indeed, along with frisks, they are the method of enforcement with which this Monitorship is most concerned. *See* Liability Order at 188 (stating that targeting young men of color “for stops” violates the constitution). As a matter of law and for purposes of evaluating statistical estimates of disparity, therefore, these omissions are unjustifiable.

We assume that the Monitor defined “enforcement” to exclude police stops for a more practical reason: only six stop reports relating to social distancing, corresponding to five separate

---

<sup>1</sup> We infer that the 78 arrests were included in the number of enforcement actions since the BWC videos were selected on the basis of the arrest data provided by the NYPD, *see* Report at 25-26, which was the same data used to calculate the probability of enforcement, *see* Report at 7.

BELDOCK LEVINE &amp; HOFFMAN LLP

Hon. Analisa Torres

December 2, 2021

Page 4

incidents, were produced over the course of the four months of study. Report at 15. In a period that prompted over 60,000 311 and 911 calls about violations of social distancing mandates, *see id.* at 7, it strains belief that police would have conducted only six social distancing stops. One obvious implication is that NYPD officers continue to underreport stops. Indeed, in prior reports, the Monitor has explicitly recognized the persistent problem of underreporting. *See, e.g.,* Monitor's Thirteenth Status Report, *Floyd* ECF No. 853-1 at 7 (“[U]ndocumented stops raise concerns about the ability to draw strong conclusions about compliance with the 14<sup>th</sup> Amendment and concerns about Blacks and Hispanics not being treated similarly to Whites when stopped by the NYPD.”).

Rather than confronting what appears to be more evidence of the NYPD's persistent underreporting of stops, however, the Monitor posits that the “issue of underreporting of Level 3 *Terry* stops does not arise in this analysis” because “there were so few Level 3 *Terry* stops.” Report at 2. But we cannot see how the Monitor supports this assessment. To know how many Level 3 *Terry* stops there actually were, it would, of course, be necessary to have complete or at least substantial reporting of those stops—yet that is precisely what is missing from the Monitor's data. The BWC footage observed by the Monitor, which represents a paltry sample of all police encounters and is also incomplete, *see infra* § 5, does little to provide a more complete tally of police stops.

If instead the six stop reports are not representative of all police stops, as we believe to be self-evident despite the Monitor's claims to the contrary, then the problem of underreporting ought to have weakened the Monitor's confidence in its assertions about overall rates of enforcement. Rather than proceeding with caution in making its determinations, though, the Monitor goes on to make statements about rates of enforcement that imply the existence of comprehensive data while omitting stops from its definition of “enforcement” altogether. *See, e.g.,* Report at 13 (“There were zero enforcement events recorded in the following Precincts”); *id.* (“Patrol Borough Staten Island as a whole appeared to avoid enforcement of social distancing orders”); *id.* at 13–14 (“In addition, the overall enforcement of social distancing by the NYPD appears to be low compared to the level of violations reported through 311 and 911”). These statements, because they fail to incorporate a critical category of enforcement actions, cannot be accepted as accurate.

### **3. The Report Commits Statistical Errors in Measuring the Relationship Between Race and Enforcement**

Our experts have also pointed out several important technical issues with the Monitor's statistical analysis of the relationship between race and enforcement. These problems are explained at greater length in our experts' declarations, but we seek to bring certain particularly troubling issues directly to the Court's attention.

First, in modeling the relationship between race and enforcement, the Report appears to omit relevant predictors of enforcement other than CFS, which would include, for instance, the number of *Terry/DeBour* stops, the total number of arrests and summons, the number of officers deployed in the precinct available to observe social distancing violations, crime rates that influence

BELDOCK LEVINE &amp; HOFFMAN LLP

Hon. Analisa Torres

December 2, 2021

Page 5

police deployments and tactics, and racial composition. *See* Fagan Decl. at ¶ 9. The Report then concludes that racial bias was not a main factor in the issuance of summonses and arrests. Missing from the model are these relevant and meaningful covariates that would provide a more precise estimate of the racial makeup of social distancing enforcement actions. *See id.* To the extent that the Monitor did control for additional variables, it failed to name those variables, which makes the test difficult to assess and impossible to replicate. The model also crucially fails to account for the fact that some precincts are more heavily exposed to police than others—a factor essential to reliably calculating the probability of enforcement. *See id.* at ¶ 7.

The Report additionally purports to run a disparate impact test to determine whether social distancing enforcement was biased. *See id.* at ¶ 8. Yet in order to run a proper disparate impact test, the Monitor would need to know which social distancing encounters did *not* result in the designated outcomes of arrests and summonses. *See id.* We appreciate that the Monitor did not have non-enforcement data, *see* Report at 14–15, but this only means that the chosen test was unlikely to yield reliable conclusions which should have cautioned the Monitor against making conclusions about disparate impact or inequalities in enforcement, including the conclusion that there was no disparate impact.

Finally, the Report measures enforcement disparities at the precinct level, thereby eliding significant racial distinctions *within* precincts.<sup>2</sup> *See* Fagan Decl. at ¶ 10. This problem is compounded by the fact that the Report organizes precincts by their “non-white” composition, rather than conducting separate analyses for the percentile Black or Hispanic, despite the large social class differences between such communities. *See id.* at ¶ 15.

#### **4. The Monitor’s Conclusions about Racial Disparity in Enforcement are Not Supported by the Data Presented**

Throughout the Report, the Monitor draws conclusions about the absence of racial disparities in social distancing enforcement, conclusions that are unsupported by the statistics reported. These include overall conclusions about the role of race in enforcement in general, as well as smaller and more concrete conclusions—for instance about racial disparities in the issuance of summonses in particular—that the Monitor reaches along the way. The Monitor consistently drew strained conclusions that enforcement was not racially disparate even where the data suggested otherwise or were at least inconclusive. These conclusions are notably at odds with the findings of a recently published peer-reviewed article that set out to address essentially the same

---

<sup>2</sup> Rather than measuring at the precinct level, the Monitor could and should have run a model showing where stops took place, and then controlling for racial composition where the enforcement actions occurred. This was what was done in the marijuana enforcement analysis from 2019, which showed that enforcement took place in minority areas after controlling for 911 and 311 calls. *See* Susan Stellin, *Is the ‘War on Drugs’ Over? Arrest Statistics Say No*, N.Y. Times (Nov. 5, 2019), <https://www.nytimes.com/2019/11/05/upshot/is-the-war-on-drugs-over-arrest-statistics-say-no.html>.



BELDOCK LEVINE &amp; HOFFMAN LLP

Hon. Analisa Torres

December 2, 2021

Page 6

research question using statistical analysis of publicly available police administrative data.<sup>3</sup> Unlike the Monitor's Report, that article concludes that "In the early months of the COVID-19 pandemic, New York City ZIP codes with a higher percentage of Black residents had significantly higher rates of COVID-19-specific criminal court summonses and public health and nuisance arrests, even after adjusting for ZIP-code-level measures of social distancing compliance."<sup>4</sup> Indeed, the analysis determined that racial inequities in the enforcement of social distancing mandates in New York City mirrored the "imprecise and discretionary nature of the stop-and-frisk program" deemed racially discriminatory in *Floyd*.<sup>5</sup> As a result, the Court should not take the Monitor's assertions as a definitive assessment of racial and ethnic disparities in social distancing enforcement in the spring and summer of 2020.

*a. The Monitor's conclusion that most enforcement did not involve racially motivated stops and frisks*

At the end of the Report's executive summary, the Monitor declares that, according to its analysis, the "vast majority" of NYPD's "social distancing enforcement" did not involve "racially motivated stops and frisks." Report at 3. As discussed above, however, stops and frisks are not included in the Report's definition of "enforcement," they appear nowhere in its assessment of the relationship between enforcement and race, and the Monitor states that the stop report data are too limited to support any conclusions, *see* Report at 16. As discussed above, the data on which the Report relies likely fails to capture anything close to the total number of Level 3 stops or other police-civilian encounters that were conducted in relation to social distancing in 2020. The Monitor's analysis therefore cannot support the definitive conclusion that enforcement did not involve racially motivated stops.

*b. The Monitor's conclusion that the correlation between race and enforcement was not due to racial discrimination*

Even while Table 3 shows a robust correlation between the percentage of non-white population in a precinct and the probability of enforcement, the Report rejects the inference that race played a role in social distancing enforcement. *See id.* at 16–17. Instead, it points to a variety of other unpersuasive reasons for this correlation. First, the Report suggests that the correlation was "likely due to the outsized influence of a few precincts with specific events that generated multiple enforcement actions." *See id.* But that suggestion is undercut by Table 3, which already

---

<sup>3</sup> *See* Sandhya Kajeepeeta, Emilie Bruzelius, and Jessica Z. Ho, et al., *Policing the Pandemic: Estimating Spatial and Racialized Inequities in New York City Police Enforcement of Covid-10 Mandates*, CRITICAL PUB. HEALTH (2021), <https://doi.org/10.1080/09581596.2021.1987387>.

<sup>4</sup> *See id.* at 10.

<sup>5</sup> *See id.*

BELDOCK LEVINE &amp; HOFFMAN LLP

Hon. Analisa Torres

December 2, 2021

Page 7

controls for the possibility of outlier effects by conducting the comparison by rank order rather than raw number.<sup>6</sup> *See id.*; *see* Glaser Decl. at ¶ 12. The Report goes on to suggest that “there are likely other variables not accounted for in this analysis that might influence both CFS and enforcement levels in the precincts,” including population density, COVID infections or deaths in the prior month, and the number of officers in a precinct, among others. *Id.* at 37. But, as far as we can tell from the Report, it never controls for these variables to confirm their influence, instead using them as grounds to deny the existence of plausible racial bias. Rather than granting the real possibility that race may have played a role in enforcement, the Report proposes alternative explanations that amount to little more than speculation.

*c. The Monitor’s conclusion that race was not a main factor in summonses*

As a descriptive matter, the data in Figure 1 and Table 4 show that the precincts with the highest non-white populations received more summonses than expected, while those with the lowest non-white precincts received fewer than expected. *See* Report at 20–21. This would seem to suggest that racial composition affected summonses. But the Report rejects that inference because it finds that these differences, between the expected and actual number of summonses, were not “statistically significant,” meaning that they would have occurred by chance more than 5 percent of the time (i.e. they had a “p-value” of more than 0.05). *See id.* at 20. The purpose of significance testing, however, is to make inferences about a total population based on a random sample of that population. *See* Glaser Decl. at ¶ 11. Significance level, or “p-value,” represents the probability that one would be committing a false positive error to infer from the sample that there was a difference between groups in the population. *See id.* Yet the data that the Report uses in Figure 1 and Table 4 are drawn not from a sample, but instead from the entire population of those who received summonses. Any observed difference, therefore, cannot be a false positive, so the analytical question should be about *substantive* significance (how big and influential is the difference?), rather than *statistical* significance. *See id.*

The Report then uses this dubious assessment of statistical non-significance to draw yet another unsupported conclusion when it states that “the racial composition of precincts [was] not a main factor in the issuance of social distance summons.” Report at 21. Statistical significance testing, even if correctly applied here, could not support this conclusion. *See* Glaser Decl. at ¶ 9. While designed to allow an affirmative claim when a difference *is* found—that is, if there is a less than 5 percent likelihood of the difference occurring by chance, one can conclude that a difference exists—statistical significance testing cannot be used to support the opposite. *See id.* In other words, a finding that there was no *statistically significant* difference between the actual and predicted number of summonses in the highest non-white decile cannot be used to show there was *no* difference. *See In re: Zolof (Sertraline Hydrochloride) Prods. Liab. Litig.*, 858 F.3d 787, 793 (3d Cir. 2017) (“A causal connection may exist despite the lack of significant findings, due to issues such as random misclassification or insufficient power.”).

---

<sup>6</sup> In fact, when analyzed by rank, the correlation is even bigger—“large” by scientific standards—and remains highly statistically significant.

BELDOCK LEVINE &amp; HOFFMAN LLP

Hon. Analisa Torres

December 2, 2021

Page 8

The conclusion is especially unwarranted in view of the fact that the p-values in several key comparisons were actually quite low, meaning that the differences would have occurred by chance only rarely. *See* Glaser Decl. at ¶ 10. The Report found, for instance, that the apparent difference between actual and expected summonses in the seven most non-white precincts (top decile) would occur by chance 7 percent of the time, only 2 percentage points shy of the conventional threshold for “statistical significance.” *See* Report at 21, Table 4. And in the third whitest decile of precincts, where actual summonses were substantially lower than predicted, the p-value was only 0.026, indicating the presence of a statistically significant relationship between race and enforcement in that decile. *See id.*

Even if the Monitor means to advance only the argument that race did not play a *main* role in enforcement, that conclusion is of limited value to the Court’s assessment of substantial compliance. The issue in this case is whether race played *any* role in enforcement outside of individual suspect descriptions. *See* Remedial Order at 17 (“When a stop is not based on a specific suspect description, however, race may not be either a motivation or a justification for the stop”). To conclude that race did not play a *main* role in the enforcement of social distancing enforcement still leaves open the undenied possibility that race influenced enforcement in impermissible ways.

*d. The Monitor’s conclusion that arrests were due to something other than race*

The Report separately concludes, on the basis of Figure 2, that social distancing arrests were “due to something other than percentage of non-White residents in a precinct.” Report at 23. But that conclusion is also unwarranted because the model includes no other variables that might explain it. *See* Fagan Decl. at ¶ 16. In the highest non-white precincts, for instance, police officers could be making more arrests for other violations or offenses and therefore would not bother with lower priority social distancing enforcement. *See id.* Omitted variables, *see infra* § 3, make Figure 2 uninterpretable. In addition, the data show that at least four deciles—5, 6, 9, and 10—did have meaningful differences, which are not reflected in the Report’s conclusion. *See* Fagan Decl. at ¶ 15.

**5. The Report’s Conclusions about the Constitutionality of Stops from Body Worn Cameras Are Not Supported by Sufficient Data**

The Report also analyzes what it describes as a random sample of BWC footage and reaches a number of conclusions, including that “the conduct by officers was overwhelmingly lawful” and that “it appears that officers enforcing social distancing orders from the City and State engaged in Level 3 *Terry* stops only rarely.” *See* Report at 37. But these conclusions rely on data that are too limited and biased to be adequately representative as well as unexplained and unverifiable judgments by the Monitor about what the videos show.

First, the sample of videos that the Monitor observed is too small and incomplete to support many of the conclusions the Report attempts to draw. *See* Fagan Decl. at ¶ 17. This problem is especially acute in the Report’s analysis of videos for social distancing summonses, 911 calls, and



BELDOCK LEVINE &amp; HOFFMAN LLP

Hon. Analisa Torres

December 2, 2021

Page 9

311 calls. Out of 520 summonses issued, a random sample of 75 were chosen, of which only 44 were recorded in their entirety. Report at 29. The Monitor's observation of videos capturing calls for service was even more limited by the availability of BWC footage, since 911 and 311 calls are not mandatory recording events. *Id.* at 25. Out of 1,629 911 calls, 150 events were selected at random, of which only 26 videos were viewed and analyzed. *Id.* at 30. Out of 60,305 311 calls, a random sample of 50 were selected, of which only two videos were viewed and analyzed. *Id.* at 35. These numbers are simply too small a sample from which to draw the definitive and weighty conclusions that the Report attempts to make, such as that "the conduct by officers was overwhelmingly lawful." *See* Report at 37.

In addition to the problem of sample size, the selection of those few videos that were observed was biased by the inconsistent and highly discretionary decisions by officers to activate their BWCs. *See* Fagan Decl. at ¶ 17. Even though arrests and summons are "mandatory activation" events, only a fraction of them were captured by BWC video recordings. *See* Report at fn. 9 (stating vaguely that subsequent NYPD analysis located video for "most" of the arrests in question, without specifying how many videos out of the total number of arrests were in fact available); *id.* at fn. 10 (same for summonses). But rather than incorporating this fact into their assessment, the Monitor simply excluded arrests or summonses that had not been captured on video from their analysis. *See id.* at 23 ("If a video for an arrest could not be located, that arrest was excluded from the sample"); *id.* at 27 ("Any summonses . . . where a video could not be found were excluded from the analysis."). This decision renders any conclusions fraught with the most basic of sampling errors. Since the sample of videos was selected on the basis of what was available, rather than randomly, it was biased by the officers' decisions about whether to activate their body-worn cameras. These decisions, moreover, may well be closely related to the professionalism of the officer and the propriety of the stop, meaning that the Monitor may have been unable to view precisely those stops most likely to present concerns. For all of these reasons, the Monitor's observations of these videos cannot be accepted as a reliable representation of police practices around social distancing enforcement.

At various points in their analysis of the BWC data, moreover, the Monitor makes unexplained claims about the nature and legality of encounters captured on video, which are then used to support conclusions about the overall legality of enforcement. For instance, in evaluating the BWC video of arrests, the Report states "there were 62 in which it was clear that there was no Level 3 encounter," without further explanation of what was captured in these 62 arrest videos. *See* Report at 28. Likewise, in evaluating BWC of summonses, the Report states that there "were only two summonses in which it appeared that a Level 3 *Terry* stop was involved," without explaining what was captured in the other 73 videos examined. *See id.* But the Monitor's conclusions should not simply be taken at their word, without explaining how they arrived at them. This issue is exacerbated by the fact that Plaintiffs have not been given access to the BWC footage on which the Report relies and therefore cannot independently assess the Monitor's conclusions.

\* \* \*

BELDOCK LEVINE & HOFFMAN LLP

Hon. Analisa Torres  
December 2, 2021  
Page 10

Each of the above problems, separately and combined, cast serious doubt on the reliability of the analysis and conclusions of the Report. Most concerning is the Monitor's pattern, throughout the Report, to arbitrarily dismiss evidence of bias and inappropriately embrace statistical nonsignificance when it implies the absence of discrimination. In view of these problems, we ask the Court to approach the Monitor's assessments of the NYPD's 2020 social distancing enforcement with appropriate caution, so that the flaws of this Report are not incorporated into the Court's future determinations of constitutional compliance.

Plaintiffs thank the Court for its time and consideration.

Respectfully submitted,



By: \_\_\_\_\_

Jonathan C. Moore  
Luna Droubi  
Marc Arena  
Rebecca Pattiz  
Katherine "Q" Adams  
Liza Batkin (*admission pending*)  
BELDOCK, LEVINE &  
HOFFMAN, LLP  
99 Park Avenue, Penthouse Suite  
New York, NY 10016  
(212) 490-0400

/s/ Omar Farah

Omar Farah  
Samah Sisay  
CENTER FOR  
CONSTITUTIONAL RIGHTS  
666 Broadway, 7th Floor  
New York, NY 10012  
Tel. (212) 614-6439

*Counsel for Floyd Plaintiffs*

BELDOCK LEVINE & HOFFMAN LLP

Hon. Analisa Torres  
December 2, 2021  
Page 11

/s/ Jin Hee Lee

Jin Hee Lee  
NAACP LEGAL DEFENSE &  
EDUCATIONAL FUND, INC.  
700 14th Street, NW, Suite 600  
Washington, DC 20005  
(202) 216-5579

Raymond Audain  
Kevin Jason  
Lauren Johnson  
Ashok Chandran  
John Cusick  
NAACP LEGAL DEFENSE &  
EDUCATIONAL FUND, INC.  
40 Rector Street, 5th Floor  
New York, NY 10006  
(212) 965-2200

/s/ Corey Stoughton

Corey Stoughton  
Steven Wasserman  
Molly Griffard  
Jennvine Wong  
THE LEGAL AID SOCIETY  
199 Water Street  
New York, NY 10038  
(212) 577-3300

*Counsel for Davis Plaintiffs*